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REMARKS ON THE THEORY OF THE PITOT TUBE

By N. W. Akimoff

Despite the great number of valuable papers and results of tests published during the past few years on the subject of that most useful instrument, the Pitot tube, the hydraulic engineering fraternity has not as yet come to any definite agreement as to whether $h = \frac{v^2}{2g}$ or $h = \frac{v^2}{g}$ is the correct formula underlying its action.

The writer has been often astonished at the extreme violence, generally characteristic of discussions on this apparently innocent subject. Take the most peaceful and gentle spirited hydraulician, convinced, for instance, that $h = \frac{v^2}{2g}$ is the right formula, and suggest to him a mere possibility of constructing an apparatus that will yield $h = \frac{v^2}{g}$. Will not his gentleness depart instantly? Will he not accuse you of ignorance, of the most malicious juggling with facts, of deliberate twisting of the most sacred opinions of classics?

As a matter of fact anyone can make a rod that will work according to $h = \frac{v^2}{g}$, although not every rod, taken at random and under all conditions, will yield this result. It has therefore occurred to the writer that the bitterness, inherent to the controversies as to whether $\frac{v^2}{g}$ or $\frac{v^2}{2g}$ is correct, is mostly based upon the fact that entirely different premises are at the botton of such discussions.

The formula $h = \frac{v^2}{g}$ is the same as $v = 0.7\sqrt{2gh}$ and it is not very difficult to build rods that will yield this result or even slightly less, instead of $v = c\sqrt{2gh}$, where c varies from 0.84 down to 0.75, as is the case in some of the rods now on the market.

It is often "assumed" that c really ought to be unity and, therefore, that smaller values of c are caused by the "suction," due to the "trailing orifice," bent back (Fig. 5, c) in the direction of the flow.

It so happens, however, that out of all means available for decreasing the value of *c*, the "trailing orifice" seems to be the least effective; experiments made both in this country and abroad show that the lowest *c* that can be secured with the trailing orifice is 0.84 and often as high as 0.92, whereas, as said before, other means enable us to bring *c* down to 0.75 and even to 0.70. Greater "reliability" is claimed by the advocates of the "trailing orifice," however, which point it is not our intention to consider in this paper; our object being to present a few considerations relative to the formula itself of the Pitot tube, and not at all either to indorse or to condemn any particular existing article now on the market.

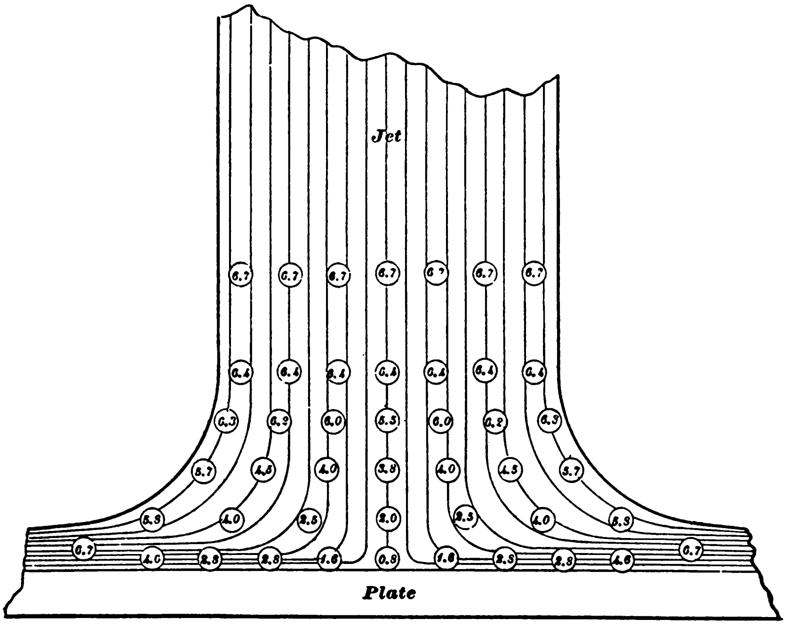

FIG. 1. STREAM LINES AND VELOCITIES OF A JET IMPINGING ON A PLATE

We shall first take up the impulse tube itself; the "static" tube will be considered later. The writer will assume that everyone is familiar with the article on "the Pitot Tube; Its Formula" by Mr. W. M. White, published in the *Journal of the Association of Engineering Societies*, August, 1901. This article, which in our time will perhaps be found somewhat unconvincing, and, possibly a trifle obsolete, contains at least one valuable feature, which will be of advantage in our discussion. In order to substantiate his opinion that in an impact tube, whose impinging surface is one of revolution, the coefficient *c*, of conversion of velocity head into static head is exactly unity, Mr. White has made many tests, the results of one of which are given in a chart (Fig. 1), taken from his paper.

A stream of water was directed against a round plate; individual velocities of separate filaments were measured and the results, in feet per second, are marked on the chart.

At the present time it would not be necessary to take all this trouble, the effects of the stream, directed both against a long, narrow strip (dam) and against a round plate, have been carefully analysed with the following most interesting results (Fig. 2, for round plate only).[1]

1. The stream lines *S* are curves of third degree, possessing this most curious property: that all cylinders, inscribed in the surface generated by each stream line are equal, so that, for instance, the volume of *A-B* is the same as that of *C-D*. The stream lines begin to diverge at a considerable distance from the plate. Of course the size of the plate proper does not in the least affect the general shape of the curves.

2. The curves of equal velocity *EV* are ellipses, located as shown and having *O* as center. Since, in general, these lines intersect the stream lines at two points, such as *F* and *G*, it is clear, that somewhere between *F* and *G* there must be a point of minimum velocity, where the corresponding ellipse is tangent to the stream line. The locus of such points of minimum velocity will be a straight line, *OH*, the angle of which with the base will be about 20°.

3. The curves of equal pressure *PP* will likewise be ellipses, but their common centre will be located below *O* and is not shown on our drawing. The curve of maximum pressures, (that is of points on each stream line, where the pressure is greatest for *that* curve) will be a hyperbola *YOY*, tangent to the plate at *O*.

Careful attention must be paid to the fact that the straight line *OH*, of minimum velocities, and the hyperbola *YOY* (of maximum pressures) do not coincide, so that minimum velocity does not mean greatest pressure, which excessive freedom with Bernoulli's theorem might lead us to think would always be the case. We readily forget that Bernoulli's formula has been established for parallel flow only and that it does not hold good for any other kind of flow; at least not in the shape in which it is given in books on hydraulics.

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[1] This construction is given in several books on advanced Hydraulics. It was originated by Professor Prasil and is laborious, although not especially difficult.

Such, then, is the correct aspect of the phenomenon of a jet impinging upon a round plate. A plate, long and narrow, would mean a somewhat different distribution of velocities etc. and all lines would appear somewhat different from those given in Figure 2. It follows, therefore, that contrary to the established assumption, the point *O* cannot possibly be considered the true impact point. Here the "static" pressure is indeed maximum, but the velocity is zero and therefore it is really the ideal point from which to determine the true static pressure, undisturbed by any "suction" or "trailing" effect.

It will of course be understood that this static pressure represents the conditions somewhere "up stream" from the plate itself; for instance in Mr. White's experiment, it simply shows the height from which the water falls. In other words we have here not the "velocity due to a certain head" but the head itself. This may be made to serve as a check, but in itself is not especially instructive or interesting in our present problem. But then, it is perfectly possible and feasible so to calibrate almost anything of this sort as to give fairly good results; only, our layout, Figure 2., or, for that matter, the "practical" sketch, Figure 1, as given by Mr. White, shows that the middle point is merely subjected to the action of dead water. We might as well have a stationary column at that point.

Any other point, in the vicinity of the curve *YOY*, of maximum pressures, will be more likely to register the effect of impact, due to velocity, in addition to static pressure at this point.

All the foregoing refers, of course, to a stream acted upon by gravity and directed downward on a horizontal plate, in other words to the condition, illustrated in Mr. White's sketch. In a pipe, with water flowing under pressure, the foregoing conclusions may require suitable corrections.

It is nevertheless quite obvious that the point which is generally assumed to be the true impact point, apparently placed against the greatest action of the stream, may prove to be at a great disadvantage, so far as the determination of velocity is concerned.

The size of the plate, as has been already stated, does not enter into our deductions; and the nozzles *B*, *C* and *D* (Figure 3, taken from Mr. White's paper) will naturally cause the same distribution of velocities and the same effect. The nozzle *B* is in the same class, because our mere desire to "catch" the energy by means of a funnel

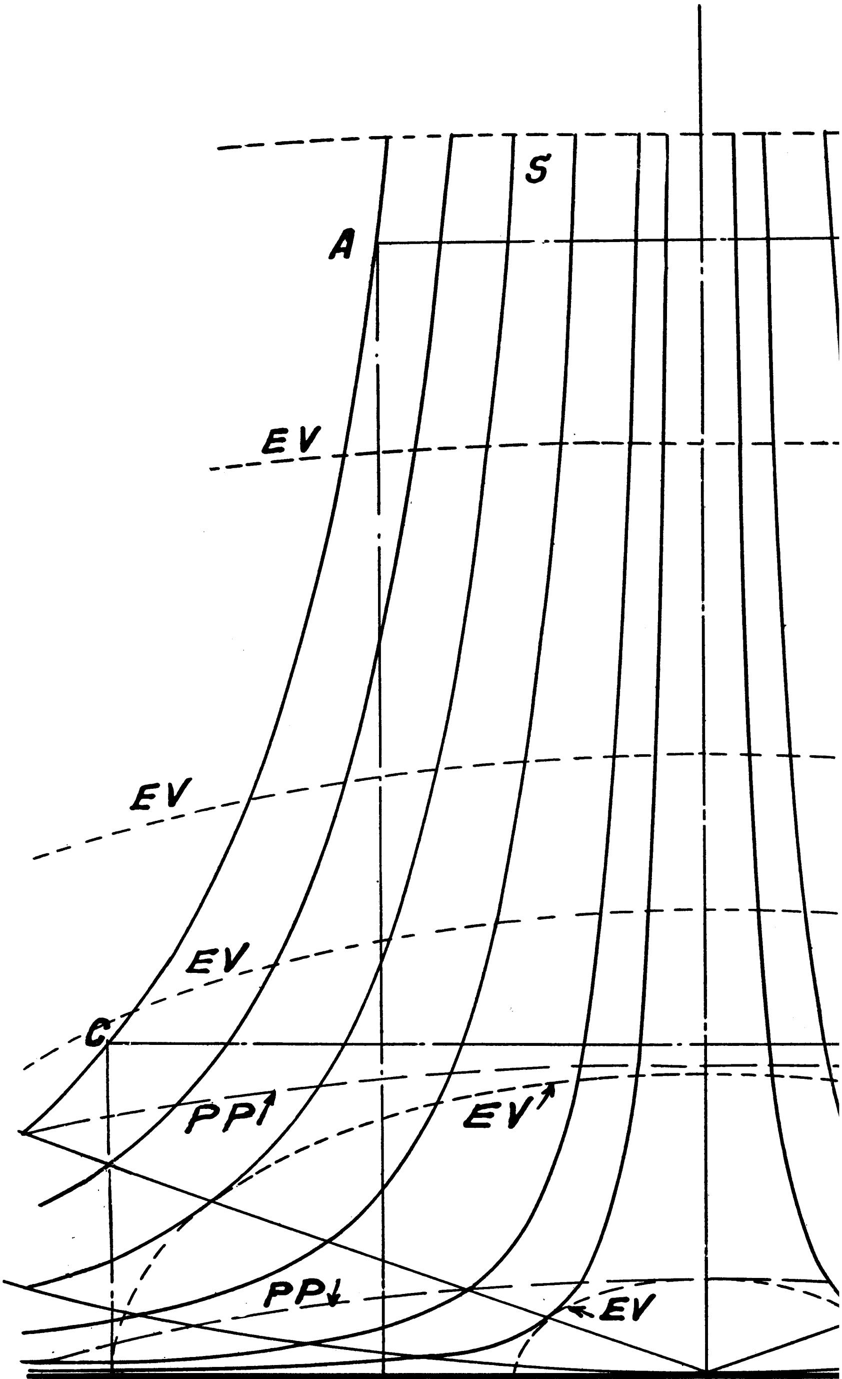

FIG. 2

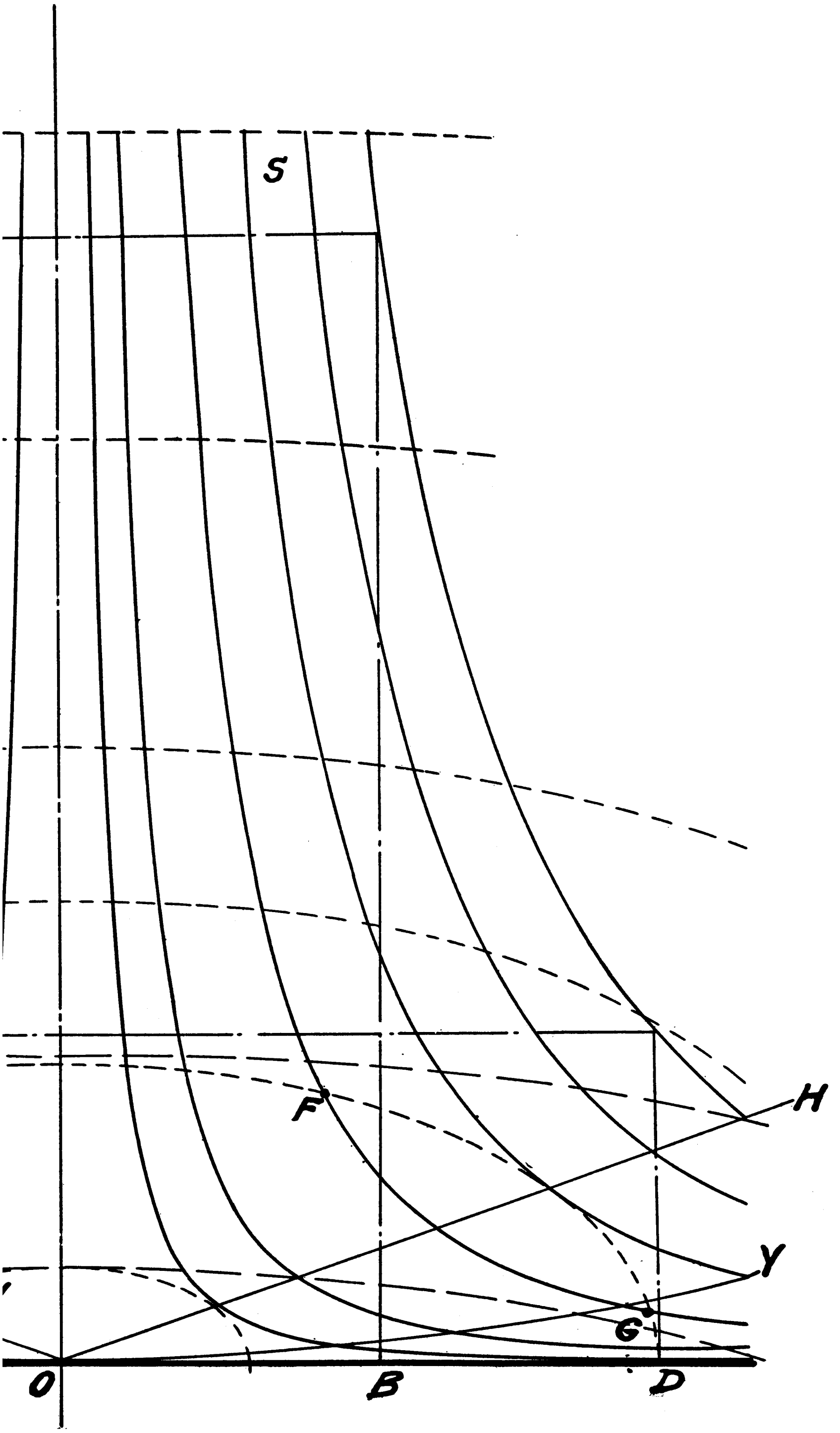

FIG. 2

shaped orifice does not in the least alter the fact of the central stream being perfectly neutral, while other stream lines may, indeed, assume a somewhat more fanciful shape than that given in Figure 2.

So far as the nozzle A is concerned (Darcy's shape), it must be remembered that, unless the nozzle itself is reasonably long, the fittings, etc., back of it should not be neglected. It will be easily seen that a large T or L piece immediately back of a very thin nozzle will cause the formation of stream lines similar to the foregoing, with the same inevitable effect, viz: the neutral stream, possessing the greatest static pressure, but inert, dynamically.

That the rod itself, beside the nozzle proper, will exert a certain influence upon the results, has been very clearly demonstrated by Professors Easby and Pardoe of the University of Pennsylvania. In their experiments a comparatively small pipe was chosen and the rod was of uniform blade-like cross-section. By means of suitable stuffing boxes the rod was made to pass completely through and extend on both sides of the pipe; so that traversing the pipe meant merely shifting the position of the orifice, and not, as in other rods, the immersion of an additional portion of the rod proper. Under these conditions the traverse curve was materially different from the one usually obtained from such small pipe (4 inch). The curve was visibly more symmetrical and thus proved, beyond doubt, the experimentors' point.

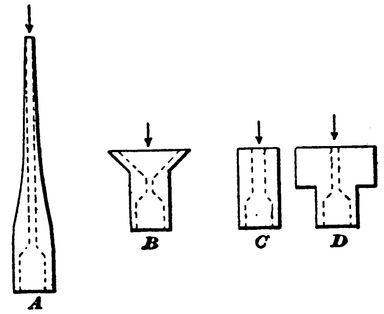

FIG. 3

The fact has been long established in theory and confirmed by experiments of all kinds, that the *force* (*not* the pressure per square inch) due to an impulse of a jet is $M \times V$, mass times velocity, where the mass is the second-weight divided by 32.2 and the velocity is in feet per second, so that the result is in *pounds*. This immediately leads to $h = \frac{v^2}{g}$, in other words to the conception of *twice the head generated by the velocity*.

That $M \times V$ is the actual force, due to an impulse, can be clearly proved by an experiment, similar to that described in Professor Merriman's *Hydraulics*, in the chapter on "Dynamic Pressure of Water," where a jet, acting upon a plate in front of an orifice, is capable of sustaining twice the head, to which the jet itself was due (Figure 4).

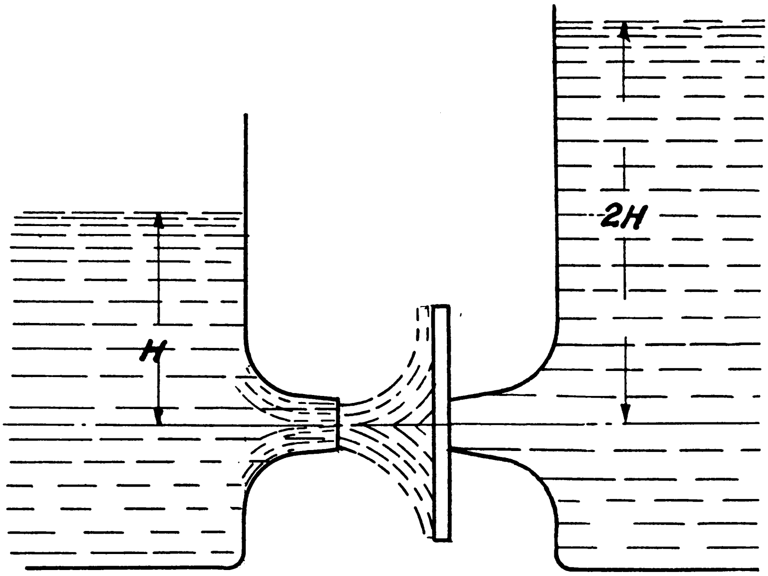

FIG. 4

Should the plate be removed the head in both vessels tends to the same value, as was almost exactly demonstrated by Professor Unwin; but does such a condition correspond to our initial problem?

In an experiment of this kind, can we possibly expect any result other than an equilibrium, subject solely to corrections due to frictional and other losses?

Zeuner, if an impartial, conservative authority is to be quoted, in his well known book on *Practical Hydraulics* (German or French), gives 1.25 as a safe value to be used in $h = \zeta \frac{v^2}{2g}$ while demonstrating that its theoretical value is $\zeta = 2$, he likewise cites a few tests made both by himself, in connection with turbine work, and by others, in experimenting on water scoops etc, where the value of this coefficient was actually 1.25. Was this because the whole jet was utilized instead of only a small central filament?

Summing up the foregoing, therefore, and without the slightest desire to question the accuracy of the numerous tests, made by well known experts, the writer does not feel that, so far, there is sufficient evidence for the fact that in a single tube $h = \frac{v^2}{2g}$ is the greatest pressure that can be secured; and certain experiments, although made in perfect good faith, rather tend to demonstrate the contrary. So much for the impulse effect.

So far as the suction is concerned, we shall begin by referring to Mr. Ferris' discussion of the same paper by Mr. White. The following are Mr. Ferris' conclusions: (1) The suction effect of the trailing orifice is very little greater than that of the plain static opening, drilled through the pipe wall; and (2) the head, obtained by him was much greater, about 60 per cent, than $\frac{v^2}{2g}$.

These remarks are very much to the point and we shall now endeavor to generalize them. To begin with, analyzing the suction effect due to the trailing orifice arrangement, one cannot help arriving to the conclusion that, owing to the comparatively low velocities, under which the Pitot tube is operated in practice, there is but little hope to secure any appreciable amount of suction by merely bending back the static tube.

Even in theory it can be proved that there is a certain minimum of velocity, beyond which no suction can possibly take place (see Zeuner's book, hereinbefore mentioned).

But there is another point not to be overlooked; the so called "static" opening, which is sometimes drilled in the pipe or else otherwise finished flush with a horizontal plane, cannot help registering *less* pressure than that corresponding to the "static" head, except, of course, for extremely low velocities. Here water is made to shoot past the opening and it is at this point that some suction effect might reasonably well be expected.

By way of illustration, beside Mr. Ferris' experiment, already mentioned, we might cite a few careful experiments made in Charlottenburg (Fig. 5 taken from Dr. Blasius' article, see *Die Turbine*, January 20, 1910). By k is meant the coefficient in $h = k\frac{v^2}{2g}$. It will be observed from c that the arrangement, which from the standpoint of "trailing" effect should yield the greatest k, actually is much inferior in this connection to the arrangement shown on sketch a, where the static pipe is finished at right angles to the flow.

It is perfectly easy further to intensify the real suction effect. It is well known that, as explained in hydrodynamics, a sudden change in the direction of velocity can cause very abrupt changes of pressure. For instance, in flowing around a sharp corner, even moderate velocities will cause zero pressures or even negative pressures (vacuum, and corresponding interrupted flow). This, then, seems to the writer to be the correct principle on which to work to secure the real suction effect, so that the value of c can be reduced to 0.70 and lower (in $v = c\sqrt{2gh}$).

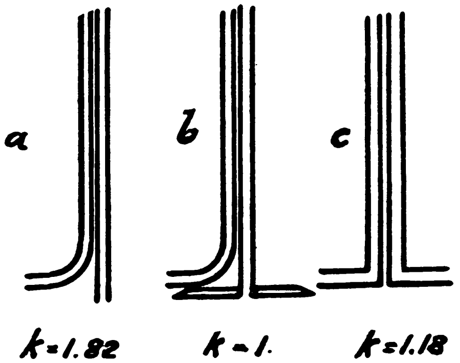

FIG. 5

This has been experimentally demonstrated by Mr. Paul Lanham, engineer in charge of pitometer division at Washington, D. C., who had a special rod made according to a sketch suggested by the writer. In fact the coefficient given by this rod, seemed to be a trifle less than 0.70, although this rod was not standardized beyond establishing the above point of giving a very low value of c.

It is the writer's hope that the foregoing may be accepted as an explanation of the fact that it is doubly possible to obtain more than $\frac{v^2}{2g}$ in Pitot tube experiments, that is to make each tube contribute its own share toward increasing the pressure difference.

So far as the open channel tests are concerned, where the Pitot tube is submerged only a few inches and is further preceded by butcher knives etc., and is followed by boats or rafts, such tests cannot be of much value except under precisely similar conditions.

Here the observed level of water is always supposed to be the same, so that there cannot be any question of registering any increased pressure as in pipe tests. Here we really trespass upon the domain of one of the most interesting and most difficult branches of hydrodynamics, the *Theory of Waves,* and should abandon all hope of "reasoning" out the problem by means of a mere misuse of Bernoulli's equation.

It is earnestly hoped that the day may not be far distant, when it will be found impossible for an engineer to do without a few elementary notions of hydrodynamics and that there may be a decided demand for well written books on the subject, which are now so sadly lacking in the engineering bibliography of this country.

The writer likewise hopes that some of the latter may contain a chapter or two on the real theory of the Pitot tube, a theory which will give a detailed explanation of the results of careful observations, made in some manner, uniformly agreed upon.